Seventy-one presents were announced as having been received since the last meeting of the Society, including, amongst others—

Kiel Observatory publications, no. ix. (E. Lamp, Comet 1891, I.; H. Kreutz, Comet 1873, V., &c.), presented by the Observatory. L. S. Fabry, Étude sur la probabilité des comètes hyperboliques et l'Origine des Comètes, presented by the author; Barlow and Bryan, Elementary Mathematical Astronomy, third edition; and Briggs and Bryan, Co-ordinate Geometry, part 1, presented by the editor.

Remarks on the Best Methods of Determining the Positions of the Planets by Observation. By David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Astronomical Journal*, No. 311 (1894 February 10), Professor Simon Newcomb has called attention to the great accuracy attainable in heliometer measures of the distance and position-angle of planets from selected comparison stars, and of the precision in the resulting places of the planet which may be reached when the comparison stars have been observed on the meridian at many different observatories, and specially when these stars have also been connected by triangulation.

The instance quoted by Professor Newcomb is afforded by the heliometer observations of *Mars* at Ascension in 1877, by which a then unexplained apparent periodic inequality in the motion of *Mars* was detected. This apparent inequality has since been shown to have arisen from an inadvertent error in the computation of the nutation in the special ephemeris of *Mars* supplied by the *Nautical Almanac* office.

Thus, to use Professor Newcomb's words, "a minute inequality, which would never have been noticed in even the best meridian observations, was brought to light, and mapped in a diagram so as to be unmistakable."

Striking as this result may be, the accuracy of the *Mars* observations is very inferior to that realised in the observations of the minor planet *Victoria* in 1889; and, pending the publication of the complete work now in the press, it may not be out of place to give the following illustrations.

It should be mentioned that the exactness of the observations has been the chief cause in the delay of publication. The results of their preliminary discussion (orally communicated to the Society at its meeting on 1893 March 10) showed, on closer examination, that the accuracy of the existing Solar Tables and the employment of seven-figure logarithms in the computation of the ephemeris from the elements were insufficient to respond to the accuracy of the observations.

With great kindness Professor Tietjen undertook the labour of computing a new ephemeris, with special perturbations and the use of eight-figure logarithms, at the office of the *Berliner Jahrbuch.* This has also involved a complete re-computation of the whole work, and the introduction of many refinements hardly applicable in the first discussion. The observations are now represented by residuals less by one-half than those previously found. This latter remark, of course, applies chiefly to the absolute errors of the planet in right ascension and declination, and not to the separate values of the parallax, because in the latter case the comparative roughness of the original ephemeris is to a great extent eliminated by the nearly simultaneous epochs of the observations in opposite hemispheres from which the parallax is deduced.

A "Set" of observations is defined as all the observations of the same class made by the same observer at the same sitting.

The various sets of observations are combined in "Groups," following each other in the order of date. The limits of these groups are so fixed as to afford in general a strong determination of the planet's right ascension, declination, and parallax from each group, the only exception to this rule being Group I., because the only observations before June 15 were a single set at the Cape on June 10, and a single set at Göttingen on June 11, both consisting only of measures of distance of the planet from the opposite stars κ and μ; and it seemed undesirable to unduly extend the limits of Group II. to include these comparatively isolated observations. Group I. thus contains only two sets of observations, and these afford a determination of the parallax, but no data to distinguish between the tabular errors in right ascension and declination.

The observations of each group are reduced to a common epoch; and in the following table are given the actual corrections, $\Delta\alpha \cos \delta$ and $\Delta\delta$, applicable to the new ephemeris, which result directly from the observations.

Group.	Period covered by the observations.	Epoch to which each group is reduced.	$\Delta\alpha \cos\delta$ O−C arc.	Weight.	$\Delta\delta$ O−C	Weight.	No. of sets.	No. of stars.	Tabular effect of Lunar perturbation. $\Delta\alpha \cos\delta$	$\Delta\delta$
II.	June 15–18	June 17·0	−0″·139	11·6	+0″·686	12·7	12	2	+2·49	−1″·22
III.	19–25	22·0	− ·153	17·8	+ ·683	19·5	22	2	+7·28	+0·74
IV.	June 26 July 2	29·0	− ·207	13·5	+ ·755	35·1	24	10	+0·98	+2·20
V.	July 3–8	July 6·0	− ·162	13·7	+ ·743	16·0	11	7	−7·33	−0·37
VI.	9–12	11·0	− ·237	12·7	+ ·746	13·9	14	3	−3·10	−2·17
VII.	15–19	18·0	− ·228	22·8	+ ·791	22·7	10	3	+7·49	+0·04
VIII.	20–22	21·5	− ·173	19·6	+ ·808	11·8	7	1	+6·93	+1·76
IX.	23–24	24·0	− ·235	20·4	+ ·873	25·1	14	4	+3·98	+2·45
X.	25–27	26·0	− ·257	17·4	+ ·832	14·3	4	1	+0·76	+2·52
XI.	July 28 Aug. 2	Aug. 1·0	− ·343	42·2	+ ·821	38·6	31	8	−1·12	+0·36
XII.	Aug. 4–9	7·0	− ·442	22·4	+ ·746	21·4	7	3	−3·15	−2·08
XIII.	10–16	13·0	− ·302	23·3	+ ·713	24·1	22	2	+6·13	−0·67
XIV.	17–21	19·0	− ·337	16·8	+ ·588	21·9	16	6	+4·84	+1·85
XV.	22–26	24·0	− ·394	13·2	+ ·505	15·4	8	1	−2·13	+1·81

These errors cannot be expressed with sufficient accuracy in terms of the elements of the planet, because of small errors in the solar tables, whence the data for the Sun's elements were derived (see report of the Cape Observatory, *Monthly Notices*, 1894 February), and also because of errors in the perturbations which may be quite sensible. But apart from the error of the lunar perturbation of the Earth, the errors of the tabular place of the planet can be developed in terms of the distance and powers of the time with all requisite accuracy.

Thus we put

$$\Delta\alpha \cos\delta = \frac{1}{\Delta}\alpha + \frac{t}{10\Delta}\beta + \frac{t^2}{100\Delta}\gamma + \frac{t^3}{10{,}000\Delta}\delta + \frac{t^4}{100{,}000\Delta} + l\mu$$

$$\Delta\delta \quad = \frac{1}{\Delta}\alpha' + \frac{t}{10\Delta}\beta' + \frac{t^2}{100\Delta}\gamma' + \frac{t^3}{10{,}000\Delta}\delta' + \frac{t^4}{100{,}000\Delta} + l\mu$$

where Δ is the distance of the planet from the Earth (mean distance of Sun from Earth being $=1$); t, the time, expressed in days, from July $18^{d}\cdot0$; α, β, γ, &c., constants to be determined; l, the effect of the tabular lunar perturbation of the Earth on the tabular places of the planet, as given in the right-hand margin of the table. (These quantities are identical with those used for the dates in question in the computation of the ephemeris.) Consequently, if L′ be the tabular value of the constant of the lunar perturbation of the Earth, the true value of that constant will be

$$L'(1+\mu).$$

Forming normals of the above type, having regard to the weights of the equations, and solving them, we obtain

A. Including all terms	... ...	$\mu = +0{\cdot}00786$
B. Excluding terms depending on t^4	...	$\mu = +\ {\cdot}00801$
C. ,, ,,	t^3 and t^4...	$\mu = +\ {\cdot}00647$

Substituting these and the values of the other unknown quantities, there result from the three solutions the following residuals in the sense O—C:

	A		B		C	
	Δα cos δ.	Δδ	Δα cos δ.	Δδ	Δα cos δ.	Δδ
	″	″	″	″	″	″
II.	+0·029	+0·001	+0·045	+0·001	0·000	+0·082
III.	− ·030	− ·017	− ·033	− ·017	− ·033	+ ·004
IV.	− ·040	+ ·026	− ·048	+ ·026	− ·028	− ·003
V.	+ ·079	·000	+ ·075	·000	+ ·093	− ·050
VI.	− ·013	− ·010	− ·013	− ·010	+ ·008	− ·056
VII.	− ·050	− ·020	− ·047	− ·020	− ·023	− ·041
VIII.	+ ·032	− ·030	+ ·037	− ·030	+ ·050	− ·033
IX.	+ ·010	+ ·029	+ ·014	+ ·028	+ ·016	+ ·037
X.	+ ·028	− ·017	+ ·032	− ·017	+ ·024	·000
XI.	− ·002	+ ·002	− ·002	+ ·001	− ·024	+ ·034
XII.	− ·051	− ·011	− ·057	− ·011	− ·085	+ ·023
XIII.	+ ·035	+ ·015	+ ·025	+ ·015	+ ·020	+ ·036
XIV.	+ ·012	− ·016	+ ·016	− ·017	+ ·020	− ·028
XV.	− ·025	+ ·011	+ ·012	+ ·010	+ ·027	− ·058

The question as to whether the development of the ephemeris may be legitimately carried beyond the terms depending upon t^2 is still under discussion, but it seems probable that the term depending on t^3 should be included. This, however, does not materially affect the immediate object in view, which is to demonstrate that the *Victoria observations show as great an advance in accuracy on the Mars observations as the Mars observations do on the meridian observations.*

As a further test of accuracy the following are the separate resulting values of the mean Solar Parallax.

Values of the Mean Solar Parallax derived from the separate groups of the Victoria Observations.

Group.	From all observations combined.	Relative Weight.	Omitting the single (small) distances.	Relative Weight.
I.	8″·751	0·9	8″·751	0·9
II.	·758	15·3	·758	15·3
III.	·807	21·2	·831	15·0
IV.	·833	36·8	·829	36·8
V.	·764	9·9	·767	9·6
VI.	·842	21·3	·777	12·2
VII.	·793	30·7	·783	23·5
VIII.	·842	30·1	·827	15·6
IX.	·727	18·6	·727	18·6
X.	·817	15·7	·731	5·0
XI.	·804	57·3	·800	51·8
XII.	·802	32·0	·753	15·9
XIII.	·826	38·5	·820	26·2
XIV.	·799	28·0	·820	21·3
XV.	·729	12·8	·700	4·2
Mean	8″·804 prob. error ±0″·0053		8″·794 prob. error ±0″·0060	

The weights of the observations of single distances have been computed according to the residuals of the individual sets after several approximations. But in the measurement of short distances with the heliometer, small systematic corrections are required, of which the true origin is not yet fully explained, and the law expressing the corrections (though closely representing the observations) is but an empirical one; thus the weights of the short distances, from the point of view of possible small outstanding systematic error, are probably too great.

The equations resulting from double distances are practically free from all systematic error, provided only that the comparison stars are, on the whole, symmetrically situated with respect to the planet, and that the planet itself is indistinguish-

able from the average star of comparison. Both these conditions are realised in the case of *Victoria.*

If the above results are combined with their nominal weights we have from the *Victoria* observations

$$\pi = 8''\cdot 800,$$

which is equivalent to giving half weight to the single observations, and the probable error is then

$$\pm 0''\cdot 0057.$$

In the observations of *Sappho* the star places were not connected by triangulation; the parallax has therefore been deduced solely from observations where the angular distance of the planet has been measured from the same pair of comparison stars, on opposite sides of the planet, at observatories in opposite hemispheres. The results are thus independent of the errors of the star places, and practically independent of any other systematic errors.

Values of the Mean Solar Parallax derived from the separate groups of the Sappho Observations.

	Group.	π	Weight.
Sept. 18–22	I.	$8''\cdot 755$	9·91
23–27	II.	·781	13·47
Sept. 28 Oct. 2	III.	·794	12·43
Oct. 10–13	V.	·815	8·61
14–18	VI.	·840	9·43
19–27	VII.	·805	14·21

Group IV., consisting of observations October 3–6, contains only the observations from the stars $t\,x$ available for our purpose. Of these, the star t is a double star (mags. 8·2 and 9·3; $s = 0''\cdot 9$, $p = 175°$), and it seems doubtful if it was similarly observed in both hemispheres. It is considered safer to reject the observations of Group IV.

The mean result is then

$$\pi = 8''\cdot 796 \text{ with prob. error } \pm 0''\cdot 007.$$

If, instead of combining the observations in groups, the parallax is derived from the twenty-two independent determinations, in which a different pair of comparison stars is used for each determination, we get

$$\pi = 8''\cdot 796 \text{ with prob. error } \pm 0''\cdot 014.$$

So that some considerable compensation of error is apparently

produced by the combination in groups. We may therefore adopt

From the *Victoria* observations,	$\pi = 8''\cdot800$	prob. error	$\pm 0''\cdot006$
„ *Sappho* „	8·796	„	$\pm$ ·012
The mean, having regard to weight,	8·799	„	$\pm$ 0·005

and it is very improbable that this value is one-hundredth part of a second of arc in error.

This value of the solar parallax, combined with Newcomb's value of the velocity of light (299,860 km.), and Clarke's value of the Earth's equatorial radius (6,378·2 km.), gives for the constant of aberration

$$20''\cdot48.$$

Chandler, in his investigation of the aberration constant from existing data, having regard to its modification by his discovery of the change of latitude, finds

$$20''\cdot50$$

for the aberration constant.

On the other hand, Loewy, by his own method, which is independent of the latitude, finds a close approximation to $20''\cdot45$; and Preston, in his discussion of the recent "Talcott" observations for change of latitude, an even smaller value. Loewy's results are independent of the change of latitude, but are seriously affected in their accidental errors (possibly to some extent systematically also) by flexure of the plane mirrors of the equatorial coudé—an instrument not so well suited as one of direct vision for the application of Loewy's beautiful method. The fine series of "Talcott" observations discussed by Preston * is wanting in early morning as well as evening observations; otherwise the result would have had a much greater weight.

Comstock's determination of the aberration constant by a modification of Loewy's method † gives $20''\cdot494 \pm 0''\cdot017$. Of all these recent values, $20''\cdot48$ seems the most probable mean; at least, one cannot conclude that the value of the solar parallax resulting from the *Victoria* and *Sappho* observations is at variance with our existing determinations of the aberration constant and the velocity of light.

Le Verrier's equations for rigorously computing the lunar perturbation of the Earth may be written in the form

$$dr = \frac{m'}{m+m'} \cdot \frac{\pi}{\sin Q} \cdot \cos s' \cdot \cos (\nu' - \odot) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$r \cdot d\nu = \frac{m'}{m+m'} \cdot \frac{\pi}{\sin Q} \cdot \cos s' \cdot \sin (\nu' - \odot) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$r \cdot ds = \frac{m'}{m+m'} \cdot \frac{\pi}{\sin Q} \cdot \sin s' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

* *U.S. Coast and Geodetic Survey Bulletin*, No. 28.
† *Astronomical Journal*, vol. xi. page 161.

where

Q is the Moon's horizontal parallax at the epoch.

s' „ „ true latitude at the epoch.

ν' „ „ „ longitude at the epoch.

r „ Earth's radius vector.

π „ mean solar parallax.

$\odot$ „ Sun's true longitude.

m and m' are the masses of the Earth and Moon.

These expressions are perfectly rigorous, but they may be developed in various ways for computing the lunar perturbation by the use of tables.

To avoid the confusion which may arise from the employment of the principal term of any development of the so-called lunar equation, we shall define the expression

$$L' = \frac{m'}{m+m'} \cdot \frac{\pi}{\sin Q_0} \quad \ldots \ldots \ldots \quad (4)$$

as "the tabular constant of the lunar perturbation of the Earth," where Q_0 is the tabular value of the mean horizontal parallax of the Moon, and the other expressions on the right-hand side of the equation are the tabular values of m' and π.

The new ephemeris of *Victoria* was first computed so that the tabular place of the planet was referred to the centre of gravity of the Earth and Moon; dr, $r \,.\, d\nu$, and $r \,.\, ds$ were then rigorously computed for every day by equations (1), (2), and (3), and the resulting values were transformed into $\Delta\alpha$ and $\Delta\delta$ so as to refer the ephemeris to the centre of the Earth.

Since the tabular values of the true radii vectores of the Earth and Moon may be regarded as exact (they are certainly so within $\frac{1}{5000}$th part of each), the true values of dr, $r \,.\, d\nu$, and $r.ds$, computed from the tabular values, may always be expressed by

$$(1+\mu)\, dr, \quad (1+\mu)\, r \,.\, d\nu, \quad (1+\mu)\, r \,.\, ds,$$

and the constant of the lunar perturbation of the Earth will be

$$L = L'(1+\mu) \,. \quad \ldots \ldots \ldots \quad (5)$$

The tabular values employed in the new ephemeris were

$$\pi = 8''\!\cdot\!880\,; \; m' = \frac{1}{83}, \text{ or } \frac{m'}{m+m'} = \frac{1}{84}\,; \; Q_0 = 3423''\!\cdot\!0.$$

Substituting these values in equation (4) we obtain

$$L' = 6''\!\cdot\!3705.$$

Hence, by equation (5),

	Solution A.	Solution B.	Solution C.
$L = L'(1+u) =$	$6''\!\cdot\!421$	$6''\!\cdot\!421$	$6''\!\cdot\!412.$

Thus the resulting constant of the lunar perturbation of the Earth will be

$$6''{\cdot}42$$

if terms higher than the square of the time are employed in determining the errors of the ephemeris, or

$$6''{\cdot}41$$

if the same development is limited to the square of the time.

Pending the definitive discussion of this point, and adopting

$$L = 6''{\cdot}42$$

we have

$$\frac{m'}{m+m'} = \frac{L \sin Q_0}{\pi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where all the quantities should be true values.

Now Q_0 is probably accurate within $\frac{1}{5000}$th part; π, as determined from the preceding observations, nearly within $\frac{1}{2000}$th part; and L (not yet definitively fixed), about $\frac{1}{600}$th part.

Substituting the known values of Q_0, and the above-mentioned values of L and π, we have, from equation (6),

$$\frac{m'}{m+m'} = \frac{1}{82{\cdot}59}$$

and

$$\text{the mass of the Moon} = \frac{1}{81{\cdot}59},$$

of which the probable error is about $\frac{1}{500}$th part.

In the face of such results it is evident that the time has arrived when astronomers should reconsider their methods of work. Future geometers are entitled to expect from the practical astronomer positions of the exterior planets five times more accurate than those at present available, and it may be well to consider the means by which such expectation can be best satisfied.

The keynote to the first advance towards this end will unquestionably be found in the spirit which has led to the success of the Astrophotographic Congress, and of the above-quoted observations of *Victoria*, viz., the international co-operation of all observatories that are occupied with astronomy of precision.

In the case of *Victoria*, twenty-two different meridian observatories co-operated in determining the places of the stars of comparison. The observatories at Göttingen, Yale, and the Cape, undertook the heliometer observation of the comparison stars, and heliometer observations of the planet itself were made

at Leipzig, Yale, Göttingen, Bamberg, and the Cape of Good Hope. Dr. Auwers also came from Berlin to take part in the observations at the Cape.

For determining important fundamental constants, like the solar parallax and the lunar equation, the highest available precision is requisite; hence the necessity for the large amount of concentrated activity on this special research. But in this case the highest precision is only required in the *relative* positions of the stars to each other and of the planet to the stars, but a like precision in the *absolute* place of the system of stars, regarded as a group, is not necessary.

The accuracy attainable in the *absolute* positions of planets is limited by the unavoidable systematic errors of our fundamental stars. At the present moment this uncertainty for any fundamental star amounts at least to $\pm 0''\cdot 3$; ultimately we may hope to reduce this uncertainty to $\pm 0''\cdot 1$. For determinations of the planet's position relative to surrounding stars, an accuracy of $\pm 0''\cdot 1$ will therefore probably suffice, and such precision can be attained for any epoch with perfect certainty by heliometer observations on three or four nights (that is to say, for the outer planets), provided that the absolute positions of the comparison stars are well enough known.

But by whatever process the position of the planet is referred to the comparison stars, and the relative position of these stars to each other is determined, their fundamental system of right ascension and declination must be ultimately referred to observations of the Sun. Thus, so far as our present knowledge and experience go, the basis of all our work must still be dependent on meridian observations with the transit circle or with some modification of that instrument in or out of the meridian.

One of the first steps, therefore, which it is desirable that astronomers should attentively reconsider is, How can the determination of absolute places of the fundamental stars be best improved? that is to say, by what method, or combination of methods, can we best arrive at the following problems?—

1. The determination of fundamental declinations, including, of course, the mean absolute latitude and the change of latitude of each fundamental observatory.

2. The elimination of the systematic error in observations of the Sun, which at present is due partly to personal error of the observer both in right ascension and declination, partly to the heat of the Sun's rays, which produce irregular expansion in the instrument employed.

3. The elimination of periodic errors in the right ascensions and declinations, or of errors depending on the brightness of the stars or on the direction of their apparent motion across the field of view.

In none of these respects does the present state of practical astronomy respond to the requirements of the problem. It is

true that every additional co-operating observatory (*especially if situated in the Southern Hemisphere*), every sound, independent method of observation, every systematic series of fundamental observations in the past or future, tends to strengthen the fundamental basis of our science.

But have these efforts in the past been thoroughly systematised so as to insure the best result from their combination? Has the work been so partitioned and systematised as to give the greatest efficiency for the labour expended? Have the instruments and observatories been so designed and employed as to afford, by their manner of construction, of employment, and of situation, the most perfect elimination of all sources of error? Have photographic and other automatic methods of registration yet been fully utilised in fundamental work?

To these and many kindred questions the answer must be in the negative.

If a new era of precision is to mark the progress of astronomy in the coming century, these questions should closely occupy the minds of astronomers in the immediate future, and the resulting suggestions be published, criticised, and made the subject of experiment.

In this way, preparation may be made for more perfect international co-operation, with better methods, better organisation, and more economy of labour in the common work.

Assuming that by some such means we shall arrive at the requisite precision in the places of the fundamental stars, it may be well to consider some of the further points raised by Professor Newcomb.

Before any satisfactory answer can be given as to the possibility of determining the position of *Mercury* and *Venus* by heliometer observations, it would be necessary to make special experiments. Having regard to the smallness of the disc of *Mercury*, the method of pointing by placing the image of the comparison star in the centre of gravity of the gibbous disc, or by bisecting the imaginary line joining the horns of the crescent, promises considerable accuracy. Such observations can only be secured in the exceptional conditions when the planet is within 2° of a star as bright as the 3rd or, in some circumstances, as the 4th magnitude. With *Venus*, especially in low latitudes, much fainter comparison stars might be similarly employed when the planet is not too near the Sun.

But it is unquestionably in observation of the outer planets that the greatest advance is possible. For *Mars*, *Jupiter*, and *Saturn*, the photographic method will probably prove far less satisfactory than heliometer observations; not that the difficulty suggested by Professor Newcomb (the fogging of the plates and the blotting out of the images of surrounding stars) may not be easily overcome, but because sharply-defined photographic images of such objects (i.e., whose limbs are capable of sharp bisection) cannot be obtained; at least, so far, this is the writer's

experience. The relative positions of *Jupiter's* satellites can, however, be determined with great accuracy by photographic methods. The minor planets, as well as *Uranus* and *Neptune*, may be photographically observed, probably with all desirable accuracy and with less labour than with the heliometer.

At present, however, there seems to exist no entirely satisfactory evidence on the latter subject, at least there is no published investigation in which the distortion of a photographic objective has been fully and satisfactorily worked out so as to show the law of distortion in terms of the distance from the centre of the plate, and also whether this distortion is the same in all radii. Bessel (*Königsberger Beobacht.* Abth. 5, p. iv.) found non-symmetric distortion in the field of the one object-glass which he fully investigated (see also *Encyclop. Britannica*, vol. xvi., Art. "Micrometer," p. 251), and some results of Jacoby's recent measurements of photographic plates appear to point to the existence of similar errors.

When this question has been fully worked out for any particular instrument (as may be comparatively easily done by photographic observation of the *Victoria* comparison stars), that instrument would probably be suitable for use, not only for the observation of *Uranus*, *Neptune*, and the minor planets, but for the triangulation or interconnection of the stars of comparison by micrometric methods.

Professor Newcomb's proposed photographic heliometer has claim to further study. It has the advantage of eliminating some of the errors due to optical distortion in the ordinary photographic method, and of personality in the observer's habit of pointing in the ordinary heliometer method. But it is very questionable if the photographic images, especially of planets, are capable of measurement with the same accuracy as the visual images. The proposed instrument has the same disadvantage as the ordinary heliometer, viz., that by one observation only one result is secured; whereas, by the ordinary photographic method the relative positions of all the available comparison stars on the plate are obtained; the proposed method also involves additional labour to that of ordinary heliometer observations, viz., the after labour of measuring the plates.

So far as can be judged from published photographs of test diagrams, there is a new form of photographic objective called the "Doppel-Anastigmate," which has been developed by Dr. C. P. Goerz, of Berlin. If this lens be adapted or modified for the photography of infinitely distant objects, it will offer great facilities for securing nearly perfect pictures over a very large field. If experiments on stars confirm this expectation, and if images of stars capable of being pointed upon with a probable error of a tenth of a second of arc at 3°, 4°, or 5° from the centre of the plate can be obtained, it would repay almost any amount of labour to investigate thoroughly the distortion errors of such a lens, and to apply it in combination with meridian

observations to a thorough triangulation and discussion of the positions of all zodiacal stars necessary for our further purposes. If such a lens can be found, the scheme can be very readily reduced to a practical form.

In observations of the Moon a decided advance seems possible, because the libration, thanks to the recent researches of Dr. Hartwig and Dr. Franz, is now known with great accuracy, and consequently the position of a well-defined spot near the Moon's centre can be referred to the limb (or, rather, to the centre of the Moon) with almost any desired precision. The position of this spot can then be referred to surrounding stars within 2° of the Moon's centre, with almost any desired precision, by means of heliometer observations. Observations by this method, however, would be limited to epochs within three or four days of full Moon, to avoid the possibility of apparent change in the position of the spot produced by change in the direction of illumination, unless, indeed, the positions of two other spots—one nearer the preceding, the other nearer the following, limb—can be also accurately referred to the Moon's centre. For observation in the Moon's first and last quarter, a great addition should be made to the list of occultation stars, and the necessary predictions of phenomena be computed for such observatories as are prepared to undertake the observations. Hardly sufficient attention appears to have been given to the accurate results obtained in this way by Dr. H. Battermann in his paper, "Beiträge zur Bestimmung der Mondbewegung und der Sonnenparallaxe aus Beobachtungen von Sternbedeckungen am sechsfüssigen Merz'schen Fernrohr der Berliner Sternwarte." The reduction by Dr. Ludwig Struve of the observations made at many different observatories, of the disappearance and reappearance of stars at the Moon's limb during the total eclipses of the Moon in 1884 and 1888, on plans proposed and elaborated by Dr. Döllen, reduces the outstanding error of the Moon's occultation diameter within very narrow limits. Future eclipses, similarly observed, will strengthen the determination. Dr. L. Struve's results, however, point clearly to the desirability of confining the observations to stars not fainter than the 8th magnitude.

Finally, we shall ere long have from the hands of Professor Newcomb a new series of homogeneous astronomical constants based on a complete discussion of all existing data, and new and more accurate tables of the planets similarly founded. Here also is the basis of a new departure!

Has not the time, therefore, arrived when practical steps should be taken to render the labour of those who undertake future work like that of Professor Newcomb, not only less laborious, but more satisfactory, than it has been in the past?

The ideal condition is that the geometer should not be required to undertake (as Professor Newcomb had to do) the reduction of vast masses of incongruous material to a homo-

geneous system—that it should be the business of practical astronomers so to arrange the methods and the partition of their work that their combined efforts shall produce all requisite results in a complete and uniformly digested shape for the purposes of those engaged in researches in astro-dynamics.

No one who has fully considered the subject, and experienced the earnest desire, which happily prevails amongst astronomers, to devote themselves to such work as that by which, with the means and opportunities at their disposal, the progress of science may be best advanced, can doubt that, by organised and combined effort, much greater things can be accomplished in the future than have been in the past.

To bring these general remarks to a practical conclusion, the following questions may be asked:—

1. Whether, in the opinion of astronomers generally, steps should be taken for a more complete and harmonious organisation and partition of the astronomical work of the world from the year 1900.

2. Are astronomers prepared to enter upon preliminary study, discussion, and experiment on the practical methods by which the art of observation may be raised to a higher level of accuracy, and its results be derived and published in a more systematic and homogeneous system?

3. If these questions are answered in the affirmative, would it be desirable to hold an International Astronomical Congress, say in 1896, to discuss and make the necessary preliminary arrangements, and then let the definitive programmė and partition of work be made at another general congress to be held in the year 1899?

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